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more than two years ago, have often seen humble bees entering these flowers. They pry or untwist the opening with their mouth organs and legs, and then pop into the barrel-shaped cavity, which they just fill.

THE DESMIDS.—O. Nordstedt has published in the part bearing date 11th Sept. of the "Lunds Universitets Arsskrift" an extensive memoir on the *Desmideæ* of S. Norway; over 260 species are described, of which some 20 or more are new. In the same journal Nordstedt describes and figures a new species of *Spirogyra* from Scania (*S. velata*).—*Journal of Botany*.

ZOOLOGY.

ENTOMOLOGY IN MISSOURI. — On pages 471–7, vol. vii, there is a flattering notice of the fifth Missouri Entomological Report, which notice, though lacking the familiar initials A. S. P., is, I infer, from the pen of one of the editors and a co-worker in the cause of economic entomology, who frequently writes over those letters. The notice contains some strictures which call for a reply:

> (1) As morphology indicates by the presence of four pairs of jointed appendages in the head, and embryology demonstrates by their early presence, four rings in the head, our author's definition of an insect as 13-jointed does not express the whole truth. (2) He should say 17-jointed, or 14-jointed, counting the head as one, in a popular report of this sort. (3) Four rings can be demonstrated in the head of an insect as easily as that the petals of a flower are modified leaves.

(1) It hardly becomes one who, if my assumption is correct, has in his own writings put forth different opinions as to how many "*typical*" joints the head of an insect is composed of, to say with such assurance, that embryology "demonstrates" that it is composed of four. The comparatively few species that have been studied embryologically will scarcely warrant our receiving such a statement as an established fact, in face of the many objections that can be brought against it. Most morphologists, believing with Sir Jno. Lubbock that there exists between Crustacea and Insecta a physiological relation analogous to that existing between water and land vertebrata, have been inclined, with Straus-Durckheim, to consider the insect head as 7-jointed, and the insect body as 20-jointed. This is a very desirable number to

give force to the idea of community of descent between these two classes, and community of structure in their exo-skeletons. But neither those who advocate 7 joints to the head, nor those who advocate 4, or 3, or 2, can claim that their particular views are demonstrated; and until they are demonstrated the advocates of the 1-jointed nature of the head have the advantage and will naturally relegate the other propositions to the limbo of pure theory. It is, moreover, difficult to conceive how those who include Arachnids and Myriopods under the term Insect can believe in any present community of structure between them.

My own view of this matter is not badly set forth in an excellent memoir by Dr. H. Schaum "On the Composition of the Head, and on the Number of Abdominal Segments in Insects,"* and to defend it properly would require a whole number of the NATURALIST, and involve a discussion of the value of the speculations so freely indulged in on this head. For this I have neither time nor inclination, and a few words must suffice. I can see no good reason why the jointed appendages of the head should be made to represent separate head segments, any more than the non-jointed appendages; and if any good reason could be given, it ought to apply to the jointed legs of the thorax as well. Yet the apodous insect larva DEVELOPS jointed legs as well as the legged larva. To me the idea that the head is composed of four joints is not a whit more tenable than the opinion that the thorax is composed of six. As Schaum has well said, it is a general law that an insect leaves the egg with the full complement of joints and none are ever added during metamorphosis. Yet many larvæ have a head without the slightest trace of a division into subjoints, and such are frequently blind or even destitute of antennæ, though their imagines possess both eyes and antennæ. Now, how can these organs be said to represent, or be developed from, joints which never had an existence?

(2) I have the satisfaction of being in most excellent company from the days of Lyonet to those of some of our best modern authors, in considering an insect 13-jointed; and to be told that I *should* "say 17-jointed or 14-jointed" does not carry that conviction which the authoritative tone might be supposed to possess. My own experience fully corroborates the views of those authors who consider that in no instance does the number of joints, in

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*Ann. and Mag. Nat. Hist; London, vol. xi, 3d ser., 1863, pp. 173-182.

true insects, exceed thirteen, though it may fall short of this number, as in the larvæ of *Hydrophilidæ* which have but twelve. This fact is plainly seen in all insects undergoing complete metamorphoses, where the head constitutes one, the thorax three and the abdomen nine joints. In some insects undergoing incomplete metamorphoses, and notably in *Libellulidæ*, an apparent tenth abdominal joint is visible; but Dr. Schaum, in the article alluded to, has very conclusively shown that what is generally mistaken for the first abdominal joint is but a posterior portion of the metathorax, and I know from conversation with, and from notes and correspondence of my late friend Walsh, who gave this question much study, that he was of the same opinion. A more or less distinct terminal subjoint is also often noticeable at the extremity of the body in many larvæ, and I especially called attention to this fact, when making the statement criticised, and cited as a prominent example, the larva of *Passalus cornutus*.*

In reality, as Erichson and Stein have proved,† this is nothing but the externally protruded anus, analogous to the anal proleg of the larvæ of many Coleoptera. The fact that dipterists have characterized the Cecidomyidous larva as differing from all other insect larvæ in having fourteen joints shows how universally the insect body is considered 13-jointed; and I have already stated my belief,‡ after examination of many species, that these larvæ form no exception to the rule of having thirteen joints and a subjoint. Strictly speaking, therefore, the body of an insect is composed of thirteen joints and a subjoint; and if we wish to employ a more arbitrary definition, the number 13 will more truly and generally apply than either 12 or 14.

(3) I have shown above that I do not believe this to be a truth; and even if it were irrefutably demonstrated that the head of an insect is composed of four elementary or embryonic joints, I should still speak of it as a single joint in referring to an insect out of the egg; for nothing would be gained, especially in a popular work, in which the abstruse in thought or expression should always be avoided, by substituting the ideal for the real. Though the petals of a flower be modified leaves, we still distinguish them as petals; and he who would attempt to do away with all the dis-

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*5th Rep., p. 7, note.

†Vergleichende Anatomie der Insecten, quoted by Schaum.

‡5th Mo. Rep., p. 114, note.

tinctive terms, as wing, fin, arm, etc., used to designate the known modifications of the same embryonic organ, would not, to my mind, cause more confusion, or be less justified, than is he who calls an insect's body 17-jointed simply because what is so palpably a single joint, was originally formed out of four embryonic joints. There is a fundamental unity of elementary structure and composition (as no one better knows than my reviewer) of all living beings; and animal and plant may alike be traced to, and have their origin in, the simple cell. Embryologically, therefore, all animals may be said to be alike, and in making our classificatory distinctions we necessarily refer to the perfected or ultimate structures.

(4) Mr. Riley also takes a back step in classification in separating the Strepsiptera from the Coleoptera, the fleas from the Diptera and the Thysanoptera from the Hemiptera. (5) It is strange if over thirty years of observation should not enable us to advance beyond Westwood's classification, admirable in 1840, but in many respects obsolete in 1873. (6) Again, our author states that embryological data "though of great value as pointing to the derivation of insects — their homologies and relations to the past — do not always subserve the best interests of classification." We would inquire what is classification but an attempt at tracing the genealogy of animals or plants?

(4) A few quotations will, I think, best refute the charge. Speaking of the Strepsiptera I distinctly say "now classed with the *Coleoptera;*" speaking of the Aphaniptera I say "now placed with the *Diptera* (5th Rep., p. 15); speaking of the Thysanoptera I distinctly state that they "may be placed with the *Pseudo-neuroptera*, though bearing strong relations to the *Hemiptera* (*ibid.*, p. 16); and I finally conclude my consideration of these osculant groups with the following sentence: — "As already stated, if separated from the other orders, these abnormal groups should, at the most, be considered as Suborders; and in reality they differ no more from the orders to which they are here referred than, for instance, the bark lice (*Coccidæ*) do from the more typical *Homoptera* from which no one thinks of separating them" (*ibid.*, p. 16).

(5) This stricture was doubtless inspired by the following quotation from that part of my Report which refers to the different systems of classification, and which I quote because it helps me to answer both the fifth and sixth strictures. "Remembering that classifications are but means to an end — appliances to facil-

itate our thought and study; and that, to use Spencer's words, 'we cannot, by any logical dichotomies, accurately express relations which, in nature, graduate into each other insensibly,' the difference of opinion becomes intelligible; and for my part I adopt that system which appears most natural, and which best promotes the object in view. It is essentially that of Westwood, given in his 'Introduction,' which has justly been called the entomologist's bible." Perhaps this language conveys the idea that I believe we have made no advance beyond Westwood's classification; but if so, it belies my meaning, and I have simply been unfortunate in expression! And as facts never become obsolete, and the "Introduction" referred to contains more facts, and fewer theories and speculations than many later published entomological works, I do not think it undeserving the homage paid to it, though it be "in many respects obsolete in 1873."

(6) I have already answered the inquiry, in my feeble way, in the above extract: and as to my opinion of the value of embryological data in classification, I shall content myself, at present, with adducing in its support the opinion of one who is infinitely better qualified to form an opinion which has weight. After referring in his last annual address, before the London Entomological Society, to Packard's "Memoir on the Embryology of Chrysopa, and its Bearings on the Classification of the Neuroptera," and to the opinions arrived at by the author, Westwood concludes as follows:—"And thus the position of the animal in the ovum is allowed to unite into one group Libellula with its active, and Hemerobius with its necromorphous pupa; and to separate widely Hemerobius and Phryganea, both with inactive pupa, which are, however, furnished with jaws of a structure, *per se*, for biting a hole in the cocoon before arriving at the fully-developed imago state. I confess that this specimen of classification founded upon embryological data does not carry to my mind conviction of its superior worth."

The accompanying figure (117) represents the male of the apple bark louse, which Riley calls *Mytilaspis pomicorticis*, regarding it as distinct from the *A. pomorum* Bouché of Europe, from the fact that the eggs of the European species are reddish-brown, while those of our species are white. Care should here be taken in ascertaining how soon after being laid the eggs are observed, as they may vary in color with the age of the embryo within. Certainly we have been unable to detect any difference between the bark louse of the apple as we have observed it in Jena, Germany, and our species, having compared numerous specimens of both. Undoubtedly our species has been imported from Europe, and it would have been the better way, we think, to regard our species as identical with the *M. pomorum* (Bouché) than to give it a new name.

Now this is not very consoling after having devoted nearly three pages to the reasons for the course pursued, in which pages every point made in the above extract is carefully met and effectually broken. It is all the less so that my reviewer has himself named species on very unsatisfactory grounds.* I have studied *Mytilaspis pomicorticis* for many years, and emphasized the fact that its eggs are never, at any stage of development, reddish-brown, and that the color of the egg is a most important character in distinguishing the closely allied *Coccidæ.* I expressly stated my belief that the European insect mentioned by Curtis, Boisduval, Taschenberg and others is identical with our's, and showed that in Europe as well as in this country it had generally been considered as Gmelin's *conchiformis* which, however, applies to a similar species found on the elm in Europe, and not to the apple tree species under consideration. No one, until last year, even so much as thought of referring our insect to Bouché's *pomorum* to which, indeed, it cannot be referred; and I regret that my views and the reasons for them are not better represented in the above-quoted stricture.

The truth is, that if, following the highest authority, we consider several very closely allied forms of Mytilaspis as specifically distinct, the European apple tree species with white eggs, which is the one imported into this country, was, up to the appearance of my last Report, erroneously referred to *conchiformis* Gmélin; and they either have a closely allied species in Europe, with reddish-brown eggs, or else Bouché's description is so false in one of the most important characters that it is valueless, and should be ignored as, indeed, it always has been. In either case, the American pomologist will appreciate more kindly than my reviewer the efforts to "brush away the cobwebs of uncertainty which have gathered around the nomenclature of the insect," and to couple an appropriate name with a description and history which cannot in future be misunderstood.

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* The orange suffers greatly from the attacks of scale insects, and, among others, of a species of Mytilaspis so closely allied to *M. pomicorticis* that no distinguishing features have yet been pointed out. The fruit which comes to the St. Louis market is often covered with the scales, and Capt. E. H. Beebe, of Geneva, Ill., last spring sent me oranges so badly infested that the scales were two and three deep; some being broad and others more linear as is the case with all the allied species. As far back as 1850 M. Ch. Delacour, in an essay on insects which attack fruit trees, considered it identical with the apple tree species, and I am unable to find any specific differences at all reliable in the dead insects. The only way in which it can be properly separated from *pomicorticis* is by critical comparison of the living males and females of both. In the Patent Office Report for 1855, Mr. Glover gave an account of this orange species and of its introduction into Florida, but without naming it. From his imperfect figures and without giving any characters of specific value, Mr. Packard (Guide, etc. p. 527) makes two new species, viz., *Aspidiotus Gloverii* and *A. citricola.*

Praying your indulgence for the length of this defence, I thank you, Messrs. editors, for the appreciation otherwise manifest in the review in question.—C. V. RILEY, *Dec.* 3, 1873.

[I should not feel called upon to notice Mr. Riley's reply to my criticism, were not the views on morphology he here reiterates in my opinion so erroneous. In reply to his section 1, I may say that the reader is referred to p. 19 of the third edition of the "Guide to the Study of Insects" for my reasons for changing my opinions as to the *number* of segments in the head of six-footed insects, and on p. 18, will be found an account of the opinions of the best authors as to the composition of the head of insects. The whole matter was settled by Savigny in 1816, and confirmed by Audouin, MacLeay, Kirby, Carus, Straus-Durckheim, Newman, Newport, Huxley and others, and by every writer on the embryology of insects. If Mr. Riley, after reading the views of these authors, and studying for himself the embryology of some insect, is content to reiterate his own and Dr. Schaum's views so confidently, I shall admire his hardihood.

2. The article of Dr. Schaum is really based on such ignorance of morphology and embryology, and is so unphilosophical in its spirit, that I wonder any one can be found to endorse it. That "what is generally mistaken for the first abdominal joint" is that joint was shown to be so by Latreille, Newman and others, and I may be pardoned for saying that I believe I have proved it by an examination of the segment in question in the larval and pupal stages of the humble bee.*

3. That the head of an insect is composed of more than one segment is simply a matter of fact; there is nothing "ideal" about it. The simple fact that the head of an insect bears four pairs of jointed appendages (*i. e.* the antennæ, mandibles, and two pairs of

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* See observations on the Development and position of the Hymenoptera, etc. Proceedings Boston Society of Natural History, 1866, vol. x, p. 279, and "Guide to the Study of Insects," p. 66.

maxillæ) indicates that it must be composed of four segments, while an examination of the head of an adult insect indicates that all the different pieces composing it cannot be referred to a single segment. Would it not be better in a "popular work" to tell the truth of the matter, and thus lead the reader to take an interest in the study of the morphology of insects, that highest department of biology, than to lead him blind-fold past some of the grandest truths in science?

4. My good friend is quite wrong in intimating that those larvæ which have heads " without the slightest trace of a division into subjoints," and are "blind or even destitute of antennæ," never had cephalic segments. If he will study Weismann's famous work on the embryology of insects, he will see that in the embryo of the flesh fly, the four segments and appendages are as distinct as in the embryo of the bee, Hydrophilus, or other beetles. The appendages become obsolete, though not wholly so, just before hatching, and Mr. Riley will probably agree with me that the differences between a "headless" maggot and a caterpillar or bee larva are probably due to differences in their mode of life. The organs are all there at the outset, in the embryo. I think Mr. Riley will set a higher value on "embryological data," after perusing the works of Rathke, Herold, Kölliker, Zaddach, Leuckart, Huxley, Claparède, and especially Weismann and Kowalevsky.

Whether my criticism on the matter of the apple bark louse was hasty and incorrect I leave to others to decide.—A. S. Packard, Jr.]

A new North American Bird.—On the 5th of July last Ludovic Kumlien, a son of Thure Kumlien, the well known ornithologist of Wisconsin, shot on Lake Koshkonong, in the central part of southern Wisconsin among a flock of the *Hydrochelidon fissipes*, a bird which he at once recognized as something entirely new to our fauna. It was a mature female and was found to contain well developed ova, though not fully grown. Mr. Kumlien, Sr., who is familiar with European forms, at once recognized it as the *Hydrochelidon leucoptera* and this determination has since been confirmed by Prof. Baird.

The *H. leucoptera* is a well known European form more common to southern Europe than farther north and has never before been known to occur on this continent. That one should be found so far in the interior of Wisconsin, a thousand miles from the coast, is somewhat remarkable, and naturally suggests the idea that this species may be found not so very rare, but that it may occur elsewhere and have been mistaken for *fissipes*, which is a cosmopolitan bird, and is found both in America and Europe. Its principal difference from the *fissipes*, consists in its white tail, and it will be well for naturalists in various parts of the country to be on the lookout for a white-tailed tern.—T. M. BREWER.

P. S. Mr. Robert Ridgway has kindly made the accompanying description of this new acquisition to our fauna:—

Wing, 7·50; tail, 2·90; culmen, ·90; tarsus, ·75; middle toe, ·65. Head, neck and lower parts to the anus, including the lining of the wings, uniform plumbeous-black; anal region, crissum, and upper tail coverts, immaculate snowy white; tail white, tinged with ashy. Mantle dark plumbeous, shading insensibly into the black of the nape; wings lighter, more hoary, plumbeous, becoming gradually white at the anterior border of the lesser covert region; primaries like the mantle, but more hoary, their shafts pure white. Bill, purplish black, the lower mandible more reddish; legs and feet deep orange-red.

This specimen is very similar to a European one in the Smithsonian Collection, but differs in having the upper tail-coverts and tail much purer white.

ECONOMIC ENTOMOLOGY.—Dr. LeConte's excellent paper, on "Hints for the Promotion of Economic Entomology in the United States," will be read with much interest, and do great good. The call for the expansion and reorganization of the Department of Agriculture is opportune and meets a similar and constantly increasing demand for such a reorganization from the agriculturists of the country and especially of the west. The few real farmers, sprinkled with the mass of lawyers, politicians, merchants and professional men, who go to make up the bodies composing our state and national legislatures, seem to have had little voice in the past, in the filling of offices created ostensibly for their benefit.

It is a burning disgrace that the agricultural interests of the country, which form the basis of our national prosperity, should have been represented in the seat of government by an untutored market-gardener like Isaac Newton; or should still be represented by one who is so little in sympathy with the progressive agricultural spirit, and who seems to give so much dissatisfaction, that hardly an agricultural journal in the land speaks a word in his favor. With a man at the head of this department, possessing large culture and scientific attainment, like Dr. LeConte, or extensive agricultural knowledge and great executive ability, like

W. C. Flagg, J. P. Reynolds or H. D. Emery of Illinois; or experience and popularity, like C. R. Dodge or Wm. Saunders (both at present connected therewith), there can be no doubt that it would be infinitely more efficient in promoting the interests for which it was created, and less open to criticism.

The agricultural interests of the country demand more attention and better representation. If our merchants lost one tithe of what our farmers annually lose from insect depredations alone, they would immediately seek and undoubtedly obtain adequate protection from the government; for the simple reason that they are organized and work as one body. The farmers, heretofore, have been disconnected—a mere rope of sand, without concerted plan or object. But at present they are building up a powerful organization which is rapidly extending its strengthening and unifying arms over the whole country. It is an organization which, if not perverted from its original aims, will soon become a very powerful lever in the promotion of the agricultural interests. May we not hope that through its instrumentality the plans and suggestions made by Dr. LeConte will at no distant day be realized!

In measures five and six (vol. vii, p. 722) as propounded in the paper, for the wholesale destruction of noxious insects, I have little confidence. Fires, lights, vessels of attractive or poisonous liquids are constantly recommended as means of counteracting the work of injurious insects; but my experience with them has been very unsatisfactory. Usually quite as many beneficial as injurious species, and very seldom any really injurious species, are thus captured; and at the best such measures are blind and inefficient ways of effecting that which can be otherwise effected with more certainty and satisfaction.—C. V. RILEY.

GEOLOGY.

REMAINS OF LAND PLANTS IN THE LOWER SILURIAN.—M. Lesquereux contributes an article to the "American Journal of Science and Arts" for Jan., 1874, in which he reports the discovery near Lebanon, Ohio, of fragments of Sigillaria in clay beds positively referrible to the Cincinnati group of the Lower Silurian. This is a remarkable discovery, as no land plants before this had been found lower down than the Lower Helderberg division of the